

XLI. *An Account of some new Experiments in Electricity, containing, 1. An Enquiry whether Vapour be a Conductor of Electricity. 2. Some Experiments, to ascertain the Direction of the Electric Matter, in the Discharge of the Leyden Bottle: with a new Analysis of the Leyden Bottle. 3. Experiments on the lateral Explosion, in the Discharge of the Leyden Bottle. 4. The Description, and Use, of a new Prime-conductor. 5. Miscellaneous Experiments, made principally in the Years 1771 and 1772. 6. Experiments and Observations on the Electricity of Fogs, &c. in Pursuance of those made by Thomas Ronayne, Esq; with a Plan of an Electrical Journal, &c. By William Henly, F. R. S.*

### SECTION FIRST.

#### AN ENQUIRY WHETHER VAPOUR BE A CONDUCTOR OF ELECTRICITY.

##### EXPERIMENT I.

Redde, May 5, 1774. **I** Insulated a glass funnel [TAB. XIII. fig. 1.] into which the streams, from a capillary tube, were directed by the electricity.

tricity. From this funnel, the electrified drops were received into a large insulated earthen dish ; across which lay a long wire ; and from its end hung a pair of light cork-balls. On working the machine (after about ninety or an hundred turns of the winch, and when fifty or sixty drops had fallen into the dish) *the balls separated*, and presently diverged, to the distance of half an inch. Then taking off the electricity, from all the bodies concerned, I blew the column of water out of the capillary tube, replaced it in the bucket, pointing towards the funnel as before, and worked the machine again, to try whether the electricity, issuing from the syphon, and passing through the air, might not electrify all the bodies, so as to separate the balls, without the jet of water ; but no such event happened. I then replaced it, with the jet falling into the funnel as before ; when it succeeded. I then tried it a second time, without the jet of water ; and it failed. I thus repeated the experiment alternately, with, and without the jet, taking off the electricity of the apparatus carefully between the trials ; till I was perfectly satisfied, that the jet of water, received into the funnel, and falling from thence into the insulated dish below, was the *medium* by which the balls, hanging from the end of the wire placed therein, became electrified. Hence I inferred, that vapour from boiling water, &c. must also be a conductor of electricity, though probably in a less degree, as being more dissipated. Having since repeated this experiment by receiving the electrified jet *immediately*

*diately into a large insulated dish, I observed the effect to be much greater.*

## EXPERIMENT II.

Having procured a tin vessel, somewhat resembling an eolipile, or a chymical retort; I placed it over a small lamp, upon my prime-conductor [TAB. XIII. fig. 2.], and filled it about half-full of boiling water. The nose of it was so situated, as to throw the electrified drops into an insulated dish, furnished with balls, as in the former experiments. After the water had been some time poured in, and I imagined enough had evaporated to have produced some drops in the neck; I examined the lip, to see whether any descended, but saw none. However, on giving the machine a turn or two, I was very agreeably surprized to see the electric streams issue exactly as from a capillary tube; and a few drops having fallen into the dish, the balls became electrical, and were attracted by my finger, at the distance of an half or three quarters of an inch. In a few turns more of the globe, they separated half an inch. I then threw out the water; and, clearing the vessel of its vapour, I remounted it upon its stand (pointing towards the dish as before), to try whether the sharp edge on the lip of the vessel would not electrify the air, sufficiently to separate the balls, as the evaporated water had done. I turned the winch a long time for this purpose; but the balls never diverged at all. I then poured in the boiling water a second time; and,

and, when the drops began to fall, the fourth turn separated the balls ; and the tenth caused them to diverge to the distance of half an inch ; and in this state of repulsion they continued a considerable time, after I had ceased to work the machine. I then took off the electricity with my finger, and again cleared the vessel of its water, &c. and, having replaced it with the point as before, I worked the machine again as usual. The air was now grown in some measure electrical ; for, at the seventh or eighth turn, the balls began to separate, and in forty turns they were about three eighths of an inch distant from each other. I then ceased to turn the winch any longer ; but had no sooner stopped, than the balls began to close, and in a very few seconds they were in contact ; whereas, in the former experiment (when the electrified drops were in the dish), on my ceasing to turn the globe, they shewed no sign at all of converging ; and, I imagine, would have remained separate a long time, if I had not taken off their electricity with my finger. I apprehend, therefore, from this experiment, *that the vapour of hot water is a conductor of electricity.*

### EXPERIMENT III.

I hung on a string, as near to the cieling of the room as I could, a pair of pith-balls, which, on working the machine a considerable time, diverged three quarters of an inch, but no wider. Then sticking into the conductor a *smoking deal-match*, and working the machine again, they presently

presently separated to the distance of two inches. The match, when placed in the same situation, and not smoking, had no such effect.

## E X P E R I M E N T . IV.

Having placed an earthen half-pint mug, upon a stand properly insulated; I fixed to a large ball of brass, which I had placed in the bottom of it; the end of a wire, six, or eight feet in length. The other end of the wire I connected with the prime conductor of a small electrical machine [TAB. XIII. fig. 3.]. Over this mug, and as near to ceiling of the room as I could, I suspended a pair of light cork balls. Then filling up the vessel with boiling water, I began to work the machine; and in fifty, or sixty turns of the winch, observed the balls to separate three eighths, or half an inch, from each other. I then took off the electricity of the bodies; emptied the vessel, and cleared it of the vapour; and having placed the apparatus in the same manner, I again worked the machine, for a longer time; but without effect. On replacing the boiling water, I succeeded as at first. At other times, when I have been able to separate the balls by the air alone, to a small distance; yet by pouring in the hot water, the vapour has presently increased their divergence from one eighth, or three sixteenths, to half an inch distance; or in that proportion, according to the state of the atmosphere with respect to dryness or moisture. In short I have repeated these kinds of experiments so often; and many times with so much success; that I can have no doubt of vapour being a conductor of electricity.

## E X P E R I M E N T V.

I insulated the rubber of my machine, and hung a pair of Mr. CANTON's balls upon the prime-conductor. I then worked the machine, and having taken off a spark, or two, to draw off the electricity naturally inherent in the rubber, &c. I observed the divergence of the balls; which was very great; in so much that the strings were bent: and on approaching the back of the rubber with a *smoking green wax taper*, just blown out, (the smoak of which was instantly attracted to it,) they diverged no wider <sup>(a)</sup>. I then took off the balls, and placed my own electrometer in its stand, upon the prime-conductor [TAB. XIII. fig. 4]; and having taken off a spark, or two, as before; I again worked the machine, to observe the repellency of the index from the stem; and found it constantly to vibrate between five, and ten degrees, of the quadrant, which was divided into fifteen. I then brought the smoking taper, within four, or five inches, of the back of the rubber, as before; and observed, that on the attraction of the smoak to it, the index presently began to rise; and in a very short time, got up to right angles. I repeated the experiment, several times, with the same success. I then tried the experiment by bringing my finger to the same distance from the rubber, and pointing towards it; but this, in many trials, had not the least effect. The taper likewise, when held at the same distance, *and not smoking*, had no effect at all. I am

(a) For this experiment, the back of the insulated rubber should be perfectly smooth; mine, is of wood; with the leather pasted down closely to it; so as to leave no points.

convinced

convinced therefore, that *the smoak* was the *medium*, which conveyed the electricity, from my hand, to the insulated rubber.

## E X P E R I M E N T VI.

I placed upon a stand, on my prime-conductor, a piece of smoking wax taper [TAB. XIII. fig 5.], when immediately, on working the machine, the smoak, from a large, and diffused volume, was much contracted ; and its motion upwards, greatly accelerated. I then took off the electricity of the conductor, and held a pair of cork balls a quarter of an inch diameter, hung on threads two and a quarter inches long, (being the nearest at hand,) perpendicularly, over the rising smoak ; and as high as I could possibly reach, standing on a chair ; this might raise the balls about five feet and an half above the prime-conductor ; when (working the machine) in a few seconds the balls separated to half an inch distance. I then removed the taper, but could not perceive that the balls were at all affected without it ; but on replacing it, they separated as before. I repeated the experiment several times, with, and without the taper, and the different effect, was constantly as above recited. I then set a tin saucer upon the stand, and placed upon the saucer, an half pint mug of boiling water [TAB. XIII. fig. 6.) ; and over this water, I presented the balls, in the rising vapour ; as I had before done in the smoak. On working the machine a few seconds, the balls diverged to the distance of one twelfth part of an inch. On removing the water, and presenting the balls as before, they ne-

ver separated at all; though I worked the machine for a longer time; but on replacing the water, in a few seconds the balls diverged, as at first. These experiments I repeated several times, and always with the same success. The smoak, therefore, in the first experiment; and the vapour of the hot water, in this last; was certainly the *medium*, which conveyed the electricity, from the prime-conductor, to the balls: and I think I may now very safely pronounce, that *SMOAK, and the VAPOUR of hot water, are absolutely conductors of electricity*; though smoak is a far better one than the vapour of hot water, and both of them are exceedingly bad ones.

Upon the question, whether vapour be a conductor of electricity; I would observe, that Dr. FRANKLIN's curious experiment, of making a visible atmosphere, round an insulated, electrified body; cannot be made, but in an exceedingly dry state of the air. The reason is obvious; but in a very dry day, I have often succeeded in the experiment; and have shewed it to several of my friends, particularly Mr. MARSHAM, and Mr. NAIRNE.

My method of doing it, is as follows: I place *the brass case* of a steel-yard weight (about two and an half inches diameter,) upon a clean, dry, stand of sealing wax: then having in readiness, *a green wax taper, with a long snuff*, I give the insulated body, a spark, from the knob of a positively charged bottle; or take one from it, by the knob of a negatively charged one (the appearance is the same in both cases); then bringing the taper, just blown out, very gently towards the insulated body,   
the

the smoak is instantly, and with a kind of violence, attracted to it; till it becomes compleatly covered with it. It remains in this situation for some seconds, when it begins to disappear, at the bottom; and proceeds gradually, till it comes to the top; where it hangs quivering, like the departing flame of a lamp. It goes off in a long thin column, which rarefies, and disperses at the top, till it occupies a great space. Observe that, in giving the spark, and bringing the taper towards the insulated body, particular care must be taken, that the air be disturbed as little as possible.

The *green wax taper*, on account of the verdegrase it contains, answers excellently, in this, and several other experiments, where smoak is required *without heat*: and I first used it, from a hint Dr. FRANKLIN was so obliging as to give me.

## S E C T I O N   S E C O N D.

Of THE DIRECTION of the electric matter, in the discharge of the LEYDEN BOTTLE.

### E X P E R I M E N T   I.

LIGHT a small wax taper, and place it, with the flame exactly between two brass balls A and B, about two inches asunder; properly introduced into the circuit [TAB. XII. fig. 8.]. Then, having given a small phial two or three turns of the globe, charging it positively, connect *the coating of it*, by a chain, with the wire of the ball A; and upon applying the knob of the phial, to the wire of the ball B, you will observe the flame to be plainly driven.

driven from it; being often blown upon the ball A, so as to blacken it with the smoak. Then charge the phial negatively, and (the apparatus remaining as before) apply the knob of the phial as at first; and you will then perceive the flame to be blown quite in the contrary direction, viz. from A towards, and often upon B, as upon Dr. FRANKLIN's principles of the Leyden bottle, it ought to be. Observe that, in this experiment it is necessary to use the least charge that can be given, just sufficient to leap the interruption in the circuit; which experience will presently determine; for if the charge be too great, the flame will be attracted as well as repelled, in the discharge of the phial; and then, nothing can be inferred from the experiment.

## EXPERIMENT II.

Charge a large jar *positively*, and insulate it; then take a long curved wire, *pointed, at both ends*, and hold it by a glass handle, so as to bring one end of the wire, half an inch, from the knob, and the other end of it, to the same distance, from the coating of the jar. You will then observe a small luminous spark, upon the point apposed to the knob of the jar, and a fine pencil, diverging from the lower point, spreading upon the coating of the jar, which will presently discharge it silently. Then charge your jar *negatively*; insulate it, and apply the wire as before; and the appearances, at the points of the wire, will be directly reversed; plainly demonstrating the direction of the electricity in the discharge of the bottle.

Another

Another very convenient and easy method, of exhibiting the phænomena of the positive and negative electricity of the inside, and outside surfaces, of a charged Leyden bottle, is by slipping a cap of metal, furnished with a ball and wire, upon the outside coating ; and mounting it upon an electric stand, in an horizontal position ; as TAB. XIV. fig. .3. ; or if the bottom of the glass be turned much upward into the body of it, a piece of wood may be worked to its shape, and cemented thereto ; and through the middle of this wood, a short tube of metal may be inserted, so as to admit the wire which is connected with the ball to pass through it ; and be brought into contact with the coating of the jar, at pleasure <sup>(b)</sup>. By this means, experiments may be made, at either end of the bottle with great facility ; and other charged or exhausted, bottles ; excited ribbons ; or other electrics : the curved pointed wire, &c. &c. may be readily applied ; and give, or receive a spark ; be attracted, or repelled ; according to the kind of electricity in the two bodies, so applied towards each other. By hanging a chain round either of the wires, and connecting it with one end of the discharging rod ; and bringing the other end of the rod, so as to leave a proper space between *that* and the ball on the wire, at the opposite end of the bottle ; the flame of a taper, &c. may be interposed ; and shew the direction of the electricity in the discharge : or a cork-ball, *hung by silk*, may play between them, in the man-

(b) For many experiments, it needs only to be connected with the curved metal, or wood, in which the charged bottle is placed.

ner described by Dr. FRANKLIN. If the balls are taken off from the wires of the bottle; the wires being pointed, and one of them placed before the globe; or a prime-conductor, electrified *positively*: the phænomena of charging the Leyden bottle will be discovered by the different appearances, at the end of the wires; as at TAB. xiv. fig. 4. If the bottle be thus placed before a conductor, electrified *negatively*; or *the insulated rubber* to a machine; the appearances, at the ends of the wires, will be reversed: as upon Dr. FRANKLIN's principles they ought to be; and thus explain his theory of the Leyden phial.

But a more simple, and beautiful analysis of the Leyden phial, hath not, I think, yet been exhibited; than the following. Let a bottle that will hold near a pint; having a long neck (about an inch in diameter,) be furnished with a small plate at the top; with a valve properly secured, after the bottle is exhausted: from which plate, a wire about one eighth of an inch in diameter, is to project a little below the neck; and terminate with a blunt end. The top is to be covered with a round brass cap, firmly fixed thereon; and made air-tight. The bottom of the bottle should be coated with tin-foil, which should be continued *three inches up the side*. This bottle will charge and discharge several times in a minute; and the tin-foil coating, will prevent the shock from affecting the hand of the operator (c). The phænomena of charging the Leyden bottle, is elegantly explained by this contrivance; and is made visible, by the end of the wire; on which, the appearances vary, according as the bottle is charged, viz. positively,

(c) The bottle being held below the edge of the coating.

or negatively ; or as the conductor, from which it charged, is electrified. TAB. XIV. fig. 5. letter A. shews such a bottle, charging *negatively*, at a conductor, loaded with *positive electricity*. Letter B. shews the same bottle, charging *positively*, at the same conductor.

TAB. XIV. fig. 6. letter C. shews the bottle charging *positively* at a conductor, *electrified negatively* ; or at the insulated rubber. Letter D. shews the same bottle, charging *negatively* at the same conductor.

### S E C T I O N   T H I R D.

Of the LATERAL EXPLOSION in the discharge of the LEYDEN BOTTLE.

#### E X P E R I M E N T   I.

HAVING made a double circuit, the first by an iron bar, one inch and an half in diameter, and half an inch thick ; the second, by four feet and an half of small chain ; on discharging a jar, containing five hundred square inches of coated surface, the electricity passed in both circuits, sparks being visible on the small chain in many places. On making the discharge of three jars, containing together sixteen square feet of coated surface, through three different chains *at the same time*, TAB. XIII. fig. 7. bright sparks were visible in them all ; and I have not the least doubt, but it would have been visible in as many more. The chains were of iron and brass, of very different lengths ; the shortest ten, or twelve inches, the

longest, many feet, in length. When those jars were discharged through the iron bar before-mentioned, together with a small chain, three quarters of a yard in length ; the whole chain was illuminated, and covered throughout with beautiful rays, like bristles, or golden hair. Having placed a large jar in contact with my prime conductor, I affixed to the coating of it an iron chain, which I also connected with a plate of metal, on which I intended to make the discharge by my discharging-rod, TAB. XIII. fig. 9. This done, I hooked another chain, much longer, and of brass, to the opposite side of the jar, and brought the end of it within eight inches and an half of the metal plate. In contact with this end, I laid a small oak-stick, eight inches long, which I covered with saw-dust of fir-wood. On making the discharge upon the plate, *both the chains* were luminous through their whole lengths ; as was also the saw-dust, which was covered by a streak of light, making a very pleasing appearance. I repeated the experiment several times. Perhaps, if I had used a bar of iron, instead of the chain first-spoken of, there might have been no light upon the second chain, or upon the saw-dust, especially as the electricity had half an inch of air to pass through, before it reached the end of the stick. But from this experiment may, I think, be inferred, the necessity of making the conductors, erected as a security to buildings, &c. from the damage of lightning, both of the best materials, and of a very sufficient substance ; and, for this purpose, perhaps nothing will be found so proper as *lead*,

lead, which will remain in the earth many centuries without any considerable decay; and the tops of chimneys being covered with it <sup>(d)</sup>, and furnished with a long, sharp-pointed rod of copper, or iron pointed with copper, which I think should extend at least five, or six feet, above the top of the chimney, or highest part of the building; a communication should be made from it by plates of lead, eight, or ten inches broad, with the lead, on the ridges, and gutters, and with the pipes which carry down the rain-water; which pipes should be continued to the bottom of the building, and there made to communicate, by means of other leaden pipe, or a plate of it, as before-mentioned, with the water in a well, the moist earth, or the main pipe which serves the house with water.

#### S E C T I O N F O U R T H.

Description and Use of a new PRIME CONDUCTOR. Contrived by Mr. HENLY, and executed by Mr. EDWARD NAIRNE.

A. TAB. XIV. fig. 7. A glass-tube, eighteen inches long, and near two inches in diameter.

B. C. Balls of Brass, with a ferule, two inches long, to each of them; which ferules are to be cemented to the ends of the tube, and made air-tight.

(d) I mention covering the tops of chimneys with lead, as a protection to the upper courses of bricks, from the effects of wind; and not as being of any essential service to the conductor, any farther than as it may assist in fixing the pointed rod, which is to be elevated above it, more securely.

One of the brass plates, which are soldered to the ferrules, hath a small hole drilled through it, by which the air is to be exhausted. It is covered by a strong valve, properly secured, and concealed by the brass ball B, or C.

D. E. Balls of brass, about five eighths of an inch in diameter, fixed upon wires, which project two inches and an half from the brass plates, at each end of the glass-tube.

F. A fine-pointed wire, to collect the electricity from the excited glass-globe, &c.

G. Supporters, of sealing-wax; upon which the luminous conductor is to be mounted.

N. B. The dots in the tube are intended to represent the appearance of the electricity in it, in the experiments described in TAB. XIV. But, when a bottle, or a large jar, is discharged through the glass-conductor, it is uniformly filled with light.

#### The use of the GLASS CONDUCTOR.

The glass-tube, thus furnished, and mounted, *being properly exhausted, and perfectly dry*, will act in all respects like one of metal; and the electrometer, being placed upon the brass ball B. will answer to the charge of a jar, or battery, exactly. But the principal use of this instrument, is to *ascertain the direction of the electric matter, as it passes through it*. And this end, it completely answers in the manner following, viz. set it with the collecting-point F. before the globe, and place the knob

knob of an uncharged bottle nearly in contact with the brass ball B. or hang a chain, &c. from thence to the table; and, on working the machine, the ball D. in the tube becomes entirely enveloped, in a dense, white atmosphere of electricity. If the point F. be brought nearly into contact with an insulated rubber, and a communication be made from the ball B. to the table; the atmosphere will be upon the ball E. in the tube. If a bottle, positively charged, be presented as in the drawing TAB. XIV. fig. 9. the appearances in the tube will be as therein delineated. But, if a bottle, charged negatively, be thus applied, the atmosphere will surround the ball E. in the tube, as in TAB. XIV. fig. 10.

CONJECTURES on these Phænomena.

It is supposed, that the impelling power of the globe, or the knob of a *positively charged bottle*, drives the particles of electricity *through the substance of the balls, wire, &c.* w th which they are in contact, with great velocity, and in a kind of straight line; but, the electricity having entered the *vacuum*, the repulsion of its particles immediately takes place, and the tube is instantly filled with light. The dense white atmosphere upon the opposite ball is supposed to proceed from the resistance of the air in the tube; a small portion of which, will, in this method of exhaustion, inevitably remain in it. And, as every particle of electricity, is supposed to be in a state of repulsion with respect to its next neighbour, the *vacuum* giving them free liberty of expanding themselves,

or standing at the greatest distance from each other ; it is imagined, that they actually do so, and will not enter the ball, and wire, at the opposite end of the conductor, *in a point, or small space*, (as they do in the open air) ; but that they entirely surround them, and enter *at all parts at the same time*, in order to their conveyance into those bodies placed at the end of the brass work to receive them.

If, instead of the brass *balls* in the tube, *points* are used ; or if a *point* be fixed at one end of the tube, and a *ball* at the other ; the effect will be precisely the same.—Note also, That the glass-conductor, for the purpose of making Dr. FRANKLIN's curious experiments, with a pointed and blunted wire, is far superior to one of metal, the electric atmosphere being so much better retained by it. By this easy and simple process, may an ocular demonstration, at all times, be given, in a dark room, and dry air, of the truth and propriety of Dr. FRANKLIN's hypothesis of the Leyden bottle.

## SECTION FIFTH.

Miscellaneous Experiments, made principally in the Years  
1771 and 1772.

### EXPERIMENT I.

IF a black silk ribband, or a piece of black silk, be laid on a quire of paper, &c. on a table, and excited by drawing over its surface sealing-wax, sulphur,

sulphur, amber, or a tube of glass with *the polish taken off by emery*; its electricity will be *positive*: whereas, if it be excited, singly, or together with a white ribband, by drawing them briskly between the fingers, it is always *negative*. ——Laying it on the paper, and drawing over its surface a rod, or tube of smooth glass, its electricity will also be negative.

#### EXPERIMENT II.

If a plate of glass, ten, or twelve inches in diameter, be excited, and placed upon the top of a box, from which a pair of light pith or cork-balls are suspended, being mounted on a stand of sealing-wax; the balls will separate, and stand repelled from each other, being electrified positively, (in a dry air), upwards of four hours. When they come into contact, on removing the glass, they diverge again, and are negatively electrified; but, on replacing it, they close. On removing it again, they separate; and thus alternately as long as any electricity remains in it (e).

If the plate of glass be placed in a frame of wood, and a light pith or cork-ball be laid on its surface; on presenting towards it the end of a finger, or the point of a pin, &c. the ball will recede from them, with a very brisk motion, and may thus be driven about upon the surface of the glass, like a feather in the air, by an excited tube, or the wire of a charged bottle. The cork-ball,

(e) For an explanation of these phænomena, see Mr. CANTON's experiments, Philosophical Transactions, Vol. LVIII. Part I. N° 53.

being

being deprived of its electricity by the pin, &c. instantly flies to that part of the glass to which it is attracted the most forcibly.

## EXPERIMENT III.

I hung on my prime-conductor a small phial, two inches in diameter, coated three inches and a quarter from the bottom. From the coating of this phial, I suspended two chains; the first, in contact with a heavy weight, placed upon a card, across which, I had ruled lines, at equal distances, TAB. XIV. fig. 1. the second chain formed a circuit, with leaden pipe, small brafs wire, small chain, &c. of one hundred and twenty feet in length. From the ball of my discharging rod, which rested on another weight (see the figure), I also hung a chain, in contact with, and completing, the circuit of one hundred and twenty feet before-mentioned; and observed, that, if my bottle was charged quite full, the electricity would, in the discharge, *pass through the long circuit*, rather than over the surface of the card, when the weights were placed at nine sixteenths of an inch asunder: but, if I charged the bottle only about half-full, the electricity would, in the discharge, pass through the long circuit, rather than over the surface of the card, though the weights were placed at the distance of only *three sixteenths* of an inch.—Query, Can there be a greater proof of the small resistance made by *metal* to the passing of the electric matter, compared with card, wood, &c. and consequently of the utility

lity of metallic conductors to buildings, ships, &c.? The same observation hath been repeatedly made, upon the effects of the natural electricity. And a remarkable instance hath lately happened, at the church of St. John, Westminster; a very exact account of which hath been taken, by Dr. WATSON, F.R.S. and J. BANKS, ESQ. F.R.S. who, I hope, will at a convenient opportunity, favour the curious in these matters with their ingenious and interesting remarks on it. The tower of this church, I am informed, hath no *pointed* metallic termination.

## EXPERIMENT IV.

Having prepared a phial, in the manner directed by Mr. LANE, for making his curious experiment; by passing a wire through the bottom, and another through the cork, so as to bring the ends of the two, within half an inch of each other, about the middle of the bottle (which was filled with water) I found, as that gentleman observed, that a slight shock of electricity discharged through it, would break the bottle. But having put a very small wire from the top, to the bottom of it, *through the water*; I discharged through it, three large jars, containing sixteen square feet of coated surface, when the whole of the small wire was exploded; but the bottle remained unhurt. If therefore a metallic conductor (being too small) should happen to be destroyed by a stroke of lightning, yet the building, &c. to which it is affixed, will probably escape uninjured.

## EXPERIMENT V.

When I strongly electrify a large prime-conductor, three feet long, and twelve inches in diameter; if a person hold in his hand a brass rod terminated by a ball, two inches in diameter, at the

distance of two inches, from the side of the conductor [TAB. XIV. fig. 2.], he will continue to draw such strong sparks as will give him a sensible shock in both his legs: but if another person at the same time present the point of a lancet, or a wire five or six inches long, nicely tapered to a point, tipped with steel, towards the conductor; though at the distance of two feet, or somewhat more, this will draw off all its electricity *silently*; and not suffer a spark to pass from thence to the brass ball: it is also observable, that if the point of the wire, or lancet, be brought nearly into contact with the prime-conductor, yet no sensation is felt in the arm, &c. of the operator: hence, I think, appears clearly the preference due to *points*, rather than round balls, or blunted ends, for the termination of the conductors erected as a security to buildings, &c. from damage by lightning: for to me, it seems probable, that the sharp point of the conductor will act upon the electric atmosphere of the cloud, and perhaps gradually and silently continue to diminish the contents, before the cloud can approach near enough to strike; and thus contribute to lessen, if not actually prevent, a stroke <sup>(f)</sup>. But should the point be struck, the consequence I suppose will not be great, and a curious instance I have now before me, which I shall beg leave to quote as follows. " About nine o'clock we had a " dreadful storm of thunder, lightning, and rain, " during which the main-mast of one of the Dutch " East Indiamen was split, and carried away by " the deck <sup>(g)</sup>; the maintop-mast and top gallant-

(f) Captain WINN's conductor, though two of the links were broken, effectually answered this intention.

(g) The stays, shrouds, &c. being all cut asunder (Dr. SO-  
LANDER informed me) as with a knife.

" mast, were shivered all to-pieces; she had an  
 " iron spindle at the main top-gallant-mast-head,  
 " which probably directed the stroke. This ship  
 " lay not more than the distance of two cables  
 " length from ours, and in all probability we should  
 " have shared the same fate, but for the electrical  
 " chain which we had but just got up, and which  
 " conducted the lightning over the side of the ship;  
 " but though we escaped the lightning, the explo-  
 " sion shook us like an earthquake, the chain at  
 " the same time appearing like a line of fire <sup>(b)</sup>: a  
 " sentinel was in the action of charging his piece,  
 " and the shock forced the musket out of his hand,  
 " and broke the rammer rod. Upon this occasion  
 " I cannot but earnestly recommend chains of the  
 " same kind to every ship, whatever be her desti-  
 " nation; and I hope that the fate of the Dutch-  
 " man will be a warning to all who shall read  
 " this narrative, against having an iron spin-  
 " dle at the mast-head." — See Capt. Cook's  
 voyage. This conductor was of copper wire,  
 three sixteenths of an inch in diameter; which  
 I am inclined to think is rather too small for  
 the purpose; I am of opinion it ought to be  
 a quarter of an inch at least: and I have been  
 informed by Dr. SOLANDER, that the point origi-  
 nally belonging to the conductor, had been  
 stolen; and that *this*, on which the lightning  
 fell, was of inferior workmanship, and not so  
 sharp; which was another great disadvantage: per-  
 haps if the wire of the chain had been larger, and

(b) It does not however appear, that the small *hempen cord*, with which the links of the chain are closely connected, or even the strings which connected them, had received the least injury from the lightning.

the point more acute, the stroke would have been much lessened; if not absolutely prevented. If, instead of those chains, *plates* of copper, three sixteenths of an inch thick, and two inches in diameter, with the edges neatly rounded off, were inserted in a groove, and continued down the main-top-gallant-mast, the main-top-mast, and part of the main-mast, into the well-hole; a communication from the mast, to the under-side of one of the decks, might be made with a plate, or rod of metal, flattened at each end; and from that rod, the conductor might be continued by plates of lead, or copper, on the under-side of the deck, and down both the outer-sides of the ship, as low as the keel, if it be thought necessary: and this method (the conductor being always in readiness, and kept perfectly in order) I should apprehend would be preferable to the chains, which are now in use. Particular care should be taken, to have all the plates, which form the conductor, as nearly as possible in contact with each other, and to fix a sharp-pointed, slender rod of copper at its summit. And for the purpose of connecting the plates, inserted in the main-top-gallant-mast, the main-top-mast, and the main-mast; if an hoop of copper were fixed in a groove of its own thickness, at the top of the main-mast; and another such hoop at the upper end of the main-top-past; perhaps they might answer this end very conveniently <sup>(i)</sup>. The learned and ingenious Dr. WATSON, F. R. S. hath, with

(i) If an objection should be made to cutting grooves in the masts, the plates of metal, which form the conductor, might readily be fastened upon the surface, and very securely.

great

great judgement and accuracy, collected from ancient history, the accounts therein recited, of electrical appearances, upon pointed bodies; as the spears of soldiers, &c. &c. <sup>(k)</sup> which have been very judiciously introduced by Dr. PRIESTLEY into his History of Electricity <sup>(l)</sup>: and I cannot but think, those accounts, furnish a very strong argument, in favour of *pointed conductors*: for had the bodies here spoken of been terminated by blunted ends, or round knobs, it is probable that many of them instead of drawing off the lightning *silently*, would have been *struck* with it; and this, being deemed a common occurrence, would have passed unnoticed, and consequently never have been recorded in history.

If pointed bodies had really the property of drawing down strokes of lightning upon themselves, I think the pillar upon Fish-street Hill, commonly called the MONUMENT, could not long have escaped. This pillar is terminated by a basin of metal, four feet and six inches in diameter. The basin is surrounded by a great number of bended plates of metal, *sharply pointed*, to represent flames of fire. From the basin, to the floor of the gallery, are fixed perpendicularly in a circular order four thick bars of iron; and in those bars are inserted twenty-eight strong hoops, and four segments of circles, of the same metal; which serve as steps from the gallery to the basin. One of these bars (being one inch thick, and five inches broad) is connected with the iron rails of the

(k) See Philosophical Transactions, vol. 48, part I. p. 210.

(l) See History and Present State of Electricity, second edition, p. 371.

stair-case, which reaches to the bottom of the building, and forms a substantial, regular conductor of metal the whole length. The monument was erected by Sir CHRISTOPHER WREN in remembrance of the fire of London, which happened in the year 1666. It was compleated by that great architect, in the year 1677; is, including the blazing urn at its summit, about two hundred and two feet in height, from the pavement; and hath never (so far as I have been able to learn) been struck by lightning. The *antennæ* and legs of the grasshopper on the Royal Exchange in Cornhill; and the tongue and tail of the dragon on the spire of Bow church in Cheapside, London, are also remarkable instances (<sup>m</sup>): indeed I have often thought it rather a favourable circumstance, that most of the lofty public buildings in this metropolis which have metallic terminations, have generally been furnished with weather-fanes, which fanes commonly end in sharp points: for had they been terminated with large round balls of metal, perhaps many more of them might long since have been demolished. Here therefore I cannot but express my earnest wishes, that, on all future occasions, where lofty public edifices are to be erected; a good pointed conductor for the lightning, may be considered by every architect, or surveyor, as an essential part of the edifice itself.

## E X P E R I M E N T VI.

I attempted to ascertain the conducting power, of different metals, in the manner following. I

(<sup>m</sup>) A great variety might be produced, but Dr. FRANKLIN hath himself rendered this unnecessary.

took

took a thick piece of paste-board, across which I ruled lines, exactly an inch asunder. Upon these lines, cross-wise, I placed the wires; which I confined by heavy weights: the edges of which weights just touched the ruled lines; leaving exactly an inch of wire between them [see TAB. XIV. fig. 1.]. The kinds I tried were, *pure gold*, *silver*, *brass*, *copper silvered*, and *iron*. They were all drawn through the same hole, except the iron, which was somewhat larger than the others. I proved them with two jars, containing eleven square feet of coated surface; and adjusted the charges, by an electrometer graduated in divisions of one tenth of an inch each, the diameter of the scale being two inches. The result was as follows:

Pure Gold	was melted at	4
Brass		6
Copper silvered		8
Pure Silver		10
Iron		10

If I gave either of the wires a division less than the number above specified, it was not melted: if I gave either of them a division more, it was *exploded*; the greater part vanishing in smoke: whereas these charges just burst them into balls.

Should any gentleman choose to repeat this experiment, I would recommend it to him to be very particular in sizing the wires; *to use a greater length*, to do it when they are all fresh drawn, to make the experiment in a very dry day, and in a room where there is no fire. With these precautions, probably there may be some difference in the result; and this method will perhaps give a

true account, of the conducting power, of the different kinds of metal.

Having lately been presented, in the most polite manner, by the celebrated Dr. LEWIS, F. R. S. &c. with six specimens of his *platina*; in as many different states: I selected the largest grains, from one of the parcels which Dr. LEWIS informed me had been repeatedly exposed to long-continued vehement fires; the most intense which he had been able to excite, or any vessels he could procure would support: and after a few small globules (consisting doubtless in great part of heterogeneous metal) had melted out, repetitions of the operation produced no further change. It was afterward boiled successively in oil of vitriol, aqua-fortis, and spirit of salt, in order to its further purification; and which indeed reduced it to a state the most pure of any that excellent chemist had been able to produce. Having ruled a line with a blunt-ended wire, over the surface of a plate of white wax;

#### EXPERIMENT VII.

I pressed in the grains of *platina* lightly, and in contact with each other; so as to form a regular line, half an inch long. At each end of the line of *platina*, and in contact with it, I placed a thick wire, with its ends nicely rounded off, and made perfectly smooth. I covered the *platina* with a piece of thick plate-glass; and then discharged through it, three jars containing sixteen square feet of coated surface: when I obtained many beautiful spherules of the *platina*. Several of them stuck to the wax, and glass; and others *imperfectly formed*, upon the edges, &c. of the grains: which proved

proved that the fusion had been compleat. This experiment I made in the presence of Mr. Ferguson, F. R. S. Mr. William Canton, Mr. Bell, and Mr. Marsham, who all acknowledged it perfectly satisfactory. Having mentioned the result of this experiment, and the method of making it, to Mr. NAIRNE; he hath since repeated it with equal success.

Being informed, by Mr. William Canton, that his brother, Mr. Thomas Canton, had, in preparing *a dried cork* for an experiment in electricity, observed some appearances which induced him to believe, that the cork had been made electrical, by only cutting it with a pen-knife, and that on examination he found it really was so; I made the following experiment.

#### EXPERIMENT VIII.

I made a long cork perfectly dry, and held one end of it very near the fire, till it began to burn. At the same time, I held a small, fine-toothed file, in the clear part of the fire, till that also, had become very dry, and rather hot. Then, having filed off the end of the cork, I applied it to a pair of neat, light pith-balls; when it attracted them both, and raised them perpendicularly, as high as the strings would permit. Having electrified the balls by excited amber, the cork would increase their divergence from one, to near two inches; or it would repel them at an inch distance, so as to drive them one inch and an half out of the perpendicular. Electrifying the balls by excited glass, these appearances were directly reversed. The cork therefore had parted with its electricity

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to the file, and plainly acted as a negative electric.

## EXPERIMENT IX.

Having neatly rounded off the corners of a piece of thin talc, about three inches square; I coated both the sides of it, within three quarters of an inch of the edges, with tin-foil, which I also rounded off at the corners. The talc, thus prepared, I observed would readily charge, without wiping, or drying the uncoated part; and the force of the shock, in the discharge, was really astonishing.

Having been shewn, by my late truly ingenious friend Mr. CANTON, an electric spark, of a very beautiful *crimson colour*, which always appeared as it was drawn *over*, or *through*, a piece of smooth wood, *at the top of the conductor-stand*, and which was supposed by some gentlemen, to be the light of electricity, *very thinly spread* upon the surface of the wood: I was exceedingly desirous to know from what cause this phænomenon really proceeded; and for that purpose made the following experiment.

## EXPERIMENT X.

I fixed between two balls, introduced into the circuit of an electric discharge, a piece of smooth wainscot, about two inches in diameter, and a quarter of an inch thick; when, upon making the discharge of a pretty large jar, I observed the wainscot to be nearly covered with the electric light, the outer parts, or edges of the light, were exceedingly thin, but the colour *very white*; as it was also in several other

other experiments, made with the same intent. I then procured a circular piece of coloured box, which was glued to the top of the stand to my prime-conductor; when, drawing strong sparks *through this wood* (of whatever colour it was), I became clearly of opinion, that *the colour of the spark varied according to its depth in the wood*; viz. if it passed upon the surface, it was white; a little below it, yellow, or orange; still lower, scarlet; and, deeper in the wood, crimson.

It having been mentioned, by some gentlemen, as their opinion, that the matter of light, and the electric matter, were the same thing<sup>(n)</sup>; I made the following experiment, in order to determine whether there was any foundation for such an opinion or not.

#### EXPERIMENT XI.

I insulated the rubber of my machine, and placed it in such a situation, that the rays of the sun, passing through the open window of my room, might fall immediately upon it; but this I observed produced no electricity. I then collected the rays into a *focus*, by means of a good convex glass, and threw them upon the back of the rubber, till it was burned quite black; but this method was attended with no better success. I then mounted one of Mr. CANTON's electrometers, furnished with very light balls, upon a stand of sealing-wax; and, having electrified them negatively, by excited amber, so as to diverge a full inch, I again

(n) Query, If this were really the case, should not electrical experiments succeed, in the most perfect manner, *in the clearest light of the sun?* and would not the evening, or night, be an exceedingly improper time to attempt making them?

collected the rays of the sun by the convex glass, and held it at such a distance as to bring the *focus* exactly upon the end of the box, which was burnt very black, and the glue in the joints melted; but the balls were not in the least affected.

Extract from Mr. BOYLE's Continuation of  
HAWKESBY's Physico-mechanical Experi-  
ments.

“ I took a large piece of good amber; and, “ having in a summer-morning (while the air was “ yet fresh) tried that it would not, without being “ excited, attract a light body I had exposed to it, “ I removed it into the sun's beams, till they had “ made it moderately hot; and then I found, as I “ expected, that it had acquired an attractive “ virtue, and that not only in one particular “ place, as it is usually observed when it is ex- “ cited by rubbing, but in divers and distant “ places at once; at any of which it would draw “ to it the light body placed within a convenient “ distance from it; so that, in this climate of “ ours, a solid body may quickly acquire an at- “ mosphere by the presence of the sun, and that “ long before the warmest part of the day.”

A N O T H E R.

“ I took a little, but thick, vessel of glass, and “ held it near the fire till it had got a *convenient* “ *degree of heat*, which was not very great, “ though it exceeded that of the amber. I found, “ as I imagined, that the heat of fire had made “ even

“ even this body attractive, as that of the sun  
 “ had made the amber.”

## REMARK.

If Mr. BOYLE, when the amber was heated, pressed it ever so lightly against his hand, in order to try its warmth, (though without the least friction), *he excited it*; and, *without this*, it was not electrical, *neither would it become so in cooling*. If the amber was *too hot*, the heated air about it would *conduct*. Therefore he was obliged to find *a convenient degree of heat*. This assertion may be proved in the following manner.

## EXPERIMENT XII.

Hold a piece of amber near the flame of a candle, till it becomes hot; then apply it to a suspended thread, and *it will not attract it*, neither will it become electrical *in cooling*; but press it ever so lightly on your hand, in order to try its heat, though without the least friction, and (*if it be not too hot*) it will be electrical, and attract it violently. Heat it again at the candle, and its electricity shall be taken quite away. Press it again gently on your finger, or hand, and the power will be restored. Apply it again to the candle, it is lost. And thus alternately. Other electrics may probably act in the same manner; as *the flame of a candle*, or *hot air*, will conduct away the electricity of glass, almost instantaneously.

## EXPERIMENT

## EXPERIMENT XIII.

Shewing Mr. NAIRNE the above-mentioned experiments; when the amber had been well-heated, and being presented to a suspended thread, having shewn no sign at all of electricity; I held it, between my thumb and fore-finger, very near the table, but not so as to touch it, that we might entirely avoid friction. He then blew against it thirty blasts, with a pair of kitchen-bellows; when presenting it to the thread, it attracted it, at the distance of one-eighth of an inch. He then blew against it, thirty blasts more, as above described; when applying it again to the thread, we saw it attracted, at half an inch distance; and, on drawing back the amber, it drew the thread after it, six, or eight inches. We repeated the experiment three times, with the like success; and are satisfied, that the amber was made electrical *by the friction of the particles of air against its surface; and not in the least by heating only.* We afterwards excited the amber, when it must have been perfectly cold, but dry, by only blowing against it as before.

The same process succeeds with glass.

## SECTION SIXTH.

Experiments and Observations on the Electricity of FOGS, &c.  
in pursuance of those made by THOMAS RONAYNE, Esq;

1771, Nov. 14. Half past eight, A. M. I find a fog, not very thick, pretty strongly electrified. The balls separate full half an inch. They keep stationary, there being little or no wind.

Nov. 19.

Nov. 19. The air is pretty strongly electrified; but the wind is so very troublesome, that I cannot determine the kind with certainty.

Dec. 2. Half past eight, A. M. a fog, moderately thick, is strongly electrified. The balls diverge half an inch; but, if they are brought near the building, they close, and open again on removing them. The mercury in the thermometer, is fifteen degrees, above the freezing point.

Dec. 18. Half past four, P. M. a moderately thick fog is strongly electrified, soon after its appearance. The balls diverge, full half an inch, and regularly close, at the approach of excited wax. The wind is troublesome; but the balls keep their distance, and at intervals very well admit trying the experiment.

1772, Jan. 5. A fog is strongly electrified positively. The balls diverge full half an inch. The air is sharp, and frosty.

Jan. 13. Nine o'clock, A. M. a fog, not very thick, is strongly electrified positively. The mercury in the thermometer, is seven degrees and an half above the freezing point. There is little or no wind.

Jan. 18. Ten o'clock, A. M. The air is pretty strongly electrified by a fall of snow.

Jan. 21. Nine o'clock, A. M. I find the air strongly electrified, during a fall of thin sleet, a mixture, of snow, and rain, very gentle. The balls separate three quarters of an inch, and remain stationary, there being little or no wind. N. B. The electricity in the air is positive.

Jan. 29.

Jan. 29. Nine o'clock, A. M. a very thick fog, and sharp frost. The air is so strongly electrified *positively*, that the balls separate *full an inch and a quarter*. There is little or no wind, and they remain stationary; so that the experiment may be made without the least danger of a mistake.

Twelve o'clock, the balls diverge as wide as at nine.

Three o'clock, P. M. the balls are exceedingly disturbed by the wind; but, blow as it will, they still keep at a great distance from each other. It freezes very hard. A quarter past four: the same as at three o'clock.

Half past five. The balls are stationary, *at three quarters of an inch distance, from each other*. The fog increases; and the rods are perfectly wet, from end to end. It is now too dark for further accurate observation.

Jan. 30. Nine o'clock, A. M. I find the air strongly electrified *positively*, in a slight frost, and thick fog. The balls separate full half an inch: they are disturbed by the wind; but it does not close them; and the experiment is tried easily. There hath been a small shower of snow, which lies thinly spread upon the houses; and I have often suspected (as I do now), that *this forms points*, and conducts the electricity faster. The electricity continued the whole day.

Feb. 4. Nine o'clock, A. M. A sharp frost, and thick fog. The air is strongly electrified *positively*: the balls diverge full three quarters of an inch. Eleven o'clock, A. M. The balls are stationary,

stationary, *at upwards of an inch distance*. They close at the approach of excited wax.

Half past two, P. M. The same as at eleven o'clock. Three o'clock. Over-cast and cloudy: the balls are very still, but shew scarce any sign of electricity.

Feb. 11. Eight o'clock, A.M. A thick fog is sensibly electrified *positively*. The balls diverge a quarter, or three eighths, of an inch. Wind S. W. and troublesome. Thermometer 38. Barometer, 29.94.

Feb. 15. Half past ten, A.M. I find a thick fog, sensibly electrical. The balls diverge five-eighths of an inch, full. Presently, after I had fixed my rod, there fell some small drops of rain. *Upon the moment of its falling*, the balls increased their divergence near, or quite, a quarter of an inch. I never saw a fog more strongly electrified when the weather was so warm, the mercury in my thermometer, in the open air, being seven and an half degrees above the freezing point. I suppose *higher in the atmosphere* it is now uncommonly cold.

Fifty minutes past two, P.M. It snows very fast. The air is now strongly electrified *positively*. The balls separate full three quarters of an inch. Wind S. W.

From the small number of experiments I have been able to make, on the electricity of the atmosphere, I cannot help being of opinion, that fogs are much more strongly electrified *in*, or *immediately after, a frost*, than at other times; and that the electricity in the fogs is often the strongest,

soon after their appearance. I also now hold it for a certain rule, that, whenever there appears a thick fog, *and the air is at the same time sharp and frosty*, that fog, is strongly electrified *positively*. Though rain may not be *an immediate*, yet I am inclined to think it is by no means a very remote consequence of electricity in the atmosphere; and, from the trifling observations I have had an opportunity to make on that subject, I have not failed to find, that, in two or three days, after I have discovered the air to be strongly electrified, (especially if that electricity continued for as long a time), we have had rain, or other falling weather, and I incline to believe, more plentifully, in proportion to the strength, and continuance, of the electricity; if not rain, snow, &c. according to the state of the atmosphere, with respect to heat and cold. If electricity be not a cause, I think it at least *a prognostic*, of falling weather. But, for further satisfaction in this particular, I would recommend it to any gentleman curious in these enquiries, and having leisure, to keep an electrical journal, upon a plan of the following kind.

Let a large book be provided, and ruled in the manner of a bill-book, used by tradesmen. The columns so ruled may contain a collection of observations in the following order: Date and day; hour, latitude and longitude, or place; divergence of the balls; kind of electricity; variation of the needle; dip of the needle; barometer, thermometer, hygrometer, wind, weather, occasional observations; to which may be added, the rain-gage, wind-gage, &c. These things being carefully noted, and

and kites being frequently raised, to the greatest heights possible, *together with a thermometer* <sup>(o)</sup>, in different states of the weather, would probably soon throw new light upon this subject, and perhaps produce discoveries, of which we now have not the least idea. From my experiments, and observations, on the electricity of fogs; I once imagined, that, whenever I saw a very thick one, and the air was at the same time sharp, and frosty; that fog, was strongly electrified positively: but I have met with several exceptions to this rule, for on Dec. 24, 25, 26, 27, 28, 29, 1772, in all which days there were thick fogs, thermometer from 36 to 33, wind constantly N. E. I could perceive no sensible electricity. Hence I conjectured, that though I could discover none *in my situation*, yet higher in the atmosphere it might probably be found in plenty, and this conjecture was presently afterward verified by Mr. NAIRNE, who observed the air to be electrical, when he stood in the golden gallery of St. Paul's cathedral, which is about two hundred and eighty feet in height, though he could discover none in the stone gallery, which is considerably lower: and the same observation hath since been made by others; and by myself, when I found the divergence of the balls to be increased, if I projected the rod, (from which they were suspended) through one of the lights, in the lanthorn; which is still higher. And it is with some pleasure that I have since observed that Dr. LIND, and Mr. BRYDONE, have made the

(o) If Lord CHARLES CAVENDISH's can be raised properly, perhaps that may be preferable to any other.

same remark in raising their electrical kites (p). I have made many observations on the electricity of the atmosphere, of which I kept a journal more than a year: but I have no desire to publish it; as I hope the curious in these matters will be favoured with a much more accurate one, by gentlemen better qualified to undertake it.

It may not perhaps be improper to observe, that in the course of my experiments upon the electricity of fogs, I have frequently observed the balls to diverge *full two inches*: but this never happened except in a thick one; when the wind was S. W., and the mercury in the thermometer, under 40. In the instances I met with, where I could discoyer no electricity in such a fog, though the mercury stood at 35 or 36, the wind was always N. E. The apparatus I used, consisted of a light rod about seven feet long, furnished with a box containing a pair of light cork balls, hung by linen threads, seven inches long. This rod was placed in a piece of wood, (over the top of one of the highest windows in the house, most remote from other buildings) properly fitted to receive it. The end of the rod, from which the balls were suspended, was elevated to an angle of about forty-five degrees. Another rod, of equal length, was provided with a tin socket, into which went a long, substantial, stick of hard sealing wax, or shell lac; which being excited, and projected out at the window, was brought near the balls; and thus readily, determined the kind of electricity in the atmosphere.

(p) See Dr. PRIESTLEY's History of Electricity, second edition, p. 333. Experiments by Mr. DE ROMAS.

But in *an open situation*, such an apparatus is unnecessary, as one of Mr. CANTON's electrometers *having light balls*, succeeds very well, when held at the distance of about two feet from the body: the back of the observer being turned towards the wind. This method also serves to determine the kind of it, when there is a much larger quantity of electricity in the atmosphere; and which would electrify the balls so strongly, if held at the end of a long rod, high in the air; that excited wax, &c. being brought under them, would not produce the least alteration in their divergence.

Notwithstanding the balls hanging from the end of my rod, *in the open air*, have in some fogs diverged full two inches; yet I have never been able to make a pair of very light ones, *hanging from an insulated conductor, in my room*, diverge in the least, by means of the electricity collected from such a fog, by a long fishing-rod, round which a fine pointed wire was twisted, and made to communicate with the conductor. On mentioning this circumstance to my worthy friend Dr. FRANKLIN; he desired me to try whether having electrified the air *in one room*, I could by introducing the end of such an insulated rod into that air, make the balls diverge, when hanging at the opposite end of it, *in another room*. I have since tried the experiment in two rooms, *separated by a passage, nine feet long*, in the following manner: I drew off the charge of a large jar, without success; but having recharged it, and drawn it off a second time; the balls hanging from the end of the rod, (upwards of twenty feet long) in the other room, diverged a full

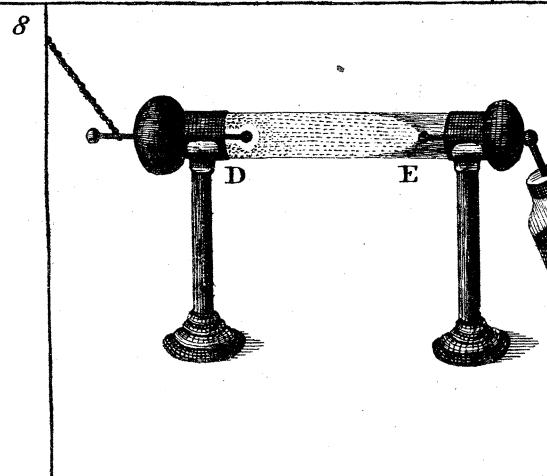
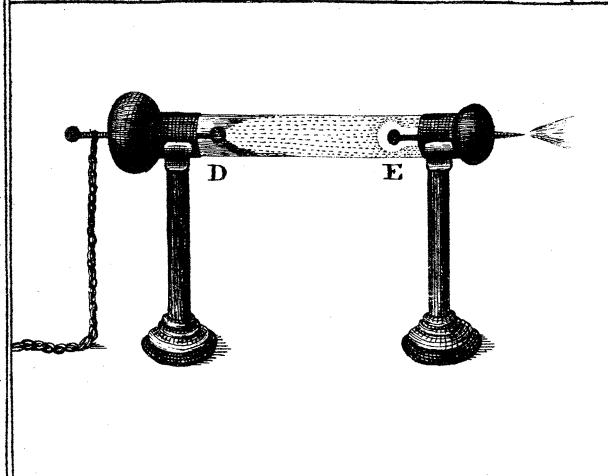
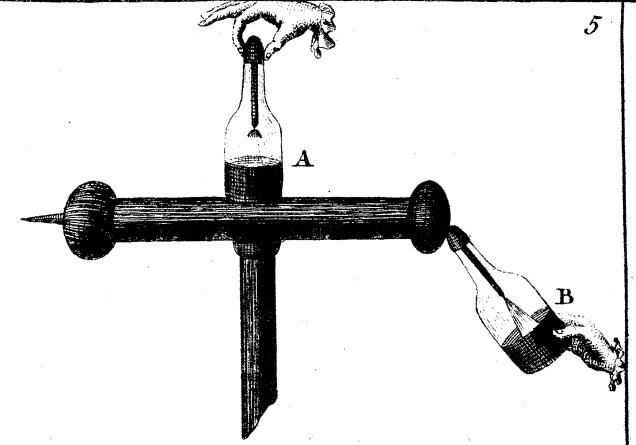
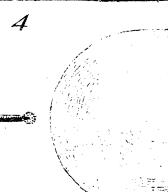
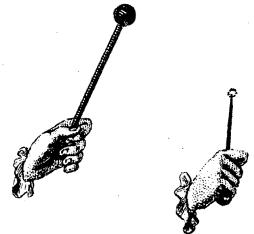
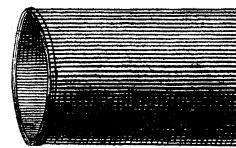
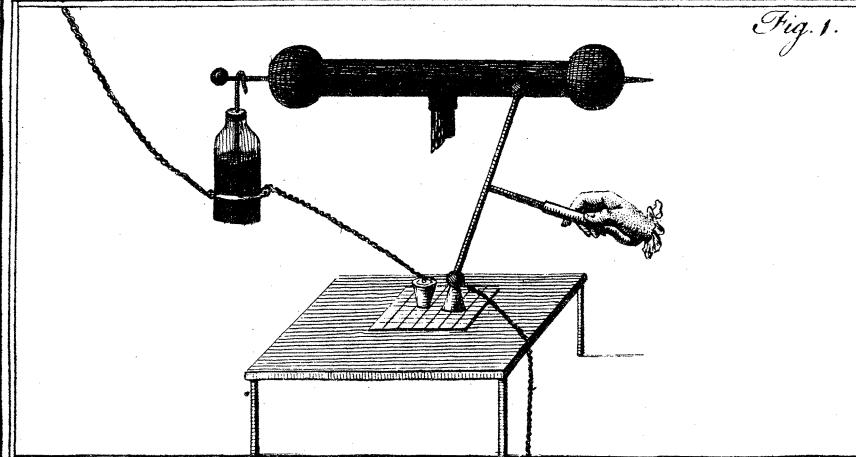
full inch; and I suppose the effect would have been greater, but there being a large fire in the room where the experiment was made, on opening the door, in order to introduce the pointed wire, which was twisted round the insulated rod, I apprehend, much of the electrified air was displaced, by the outer air pressing in upon it; and driving it to the chimney, &c.

A pair of balls hanging from my hand, near the end of the rod, *in the electrified air*, diverged one inch and an half: but being held near the other end of the rod, *in the unelectrified air*; they diverged only half an inch. I then insulated the rubber of my machine, and stuck a long sharp-pointed needle in the back of it. Then hanging a chain from my prime-conductor to the table, I began to turn the winch; when the air of the room, the end of the rod, &c. were presently affected: and the balls at the opposite end of it, in the other room, soon separated considerably more than an inch.

I cannot prevail on myself to quit this pleasing, yet difficult subject, without expressing a wish, that an electrical machine may be constructed, to work from ten, to fifty large cylinders, furnished with a prime-conductor, batteries, and other apparatus, proportionably large; the whole to be inclosed by brick walls, having flews quite round it, (like an hot-house, for botanic purposes) to keep the air in the room in a due temperature for experiment. With such a machine as this, properly managed; new, unexpected, and wonderful discoveries might be

be made: and, to use the words of my learned and ingenious friend Dr. PRIESTLEY, they are not philosophers who think no advantages could be gained by it. Electrical bells should also be set up, to give notice of electricity, in the atmosphere; and by a pair of light pith or cork balls, hanging by linen threads from the apparatus, the kind, continuance, and changes of the electricity should be carefully noted. It would also be proper to put out occasionally, a long stick, or the strongest parts of a fishing-rod, having a box, with a pair of light cork balls hanging from thence, at the end of it, which would discover much smaller degrees of electricity in the air, than is sufficient to ring bells; and by these, the kind of it may be readily ascertained. Such a course of experiments as I have recommended in this paper, would soon throw new light upon the subject of electricity; hitherto, I believe, but little understood; though so interesting to mankind, and so highly deserving the nicest investigation, of the most curious enquirers into nature. But these pursuits can be properly attended to, by those only, who are gentlemen of fortune, and leisure: and could such be prevailed on to undertake them, I have not the least doubt but the exquisite knowledge of *this secret part* of the operations of nature, which they would soon attain in the practice, would prove an ample reward of their labours; an honour to their country, and perhaps a benefit to the whole human race.

Fig. 1.



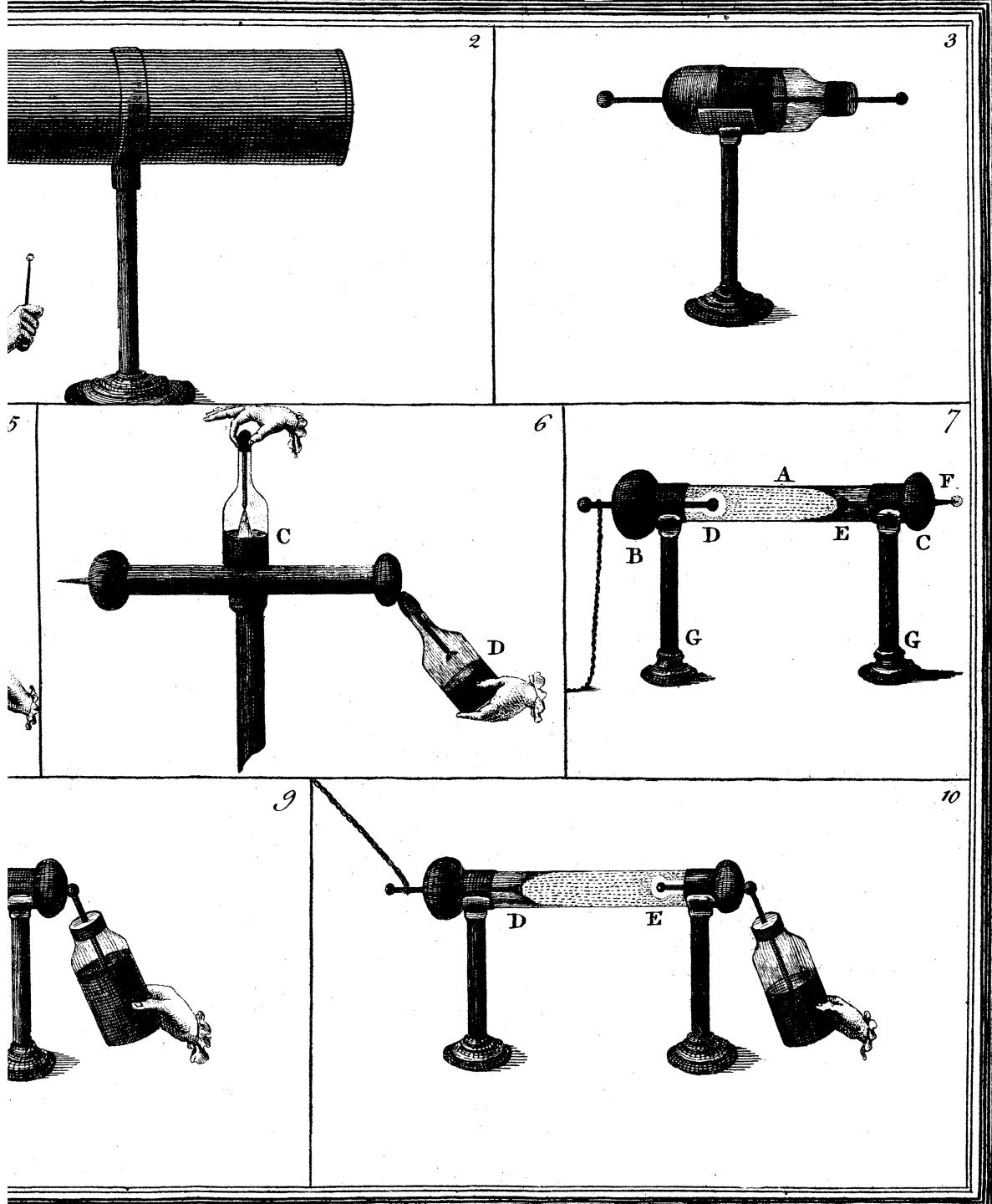
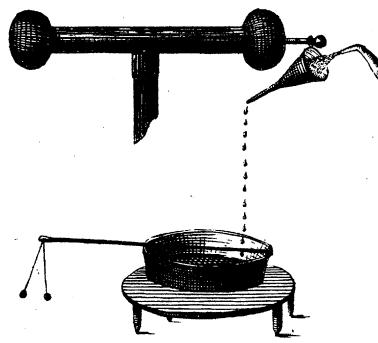
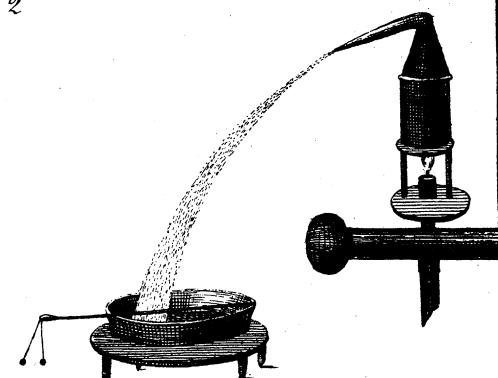


Fig. 1.



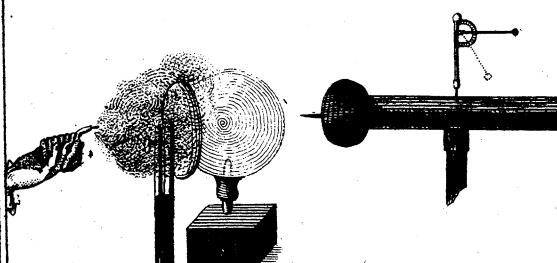
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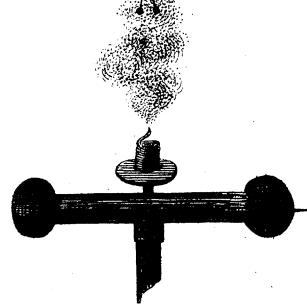
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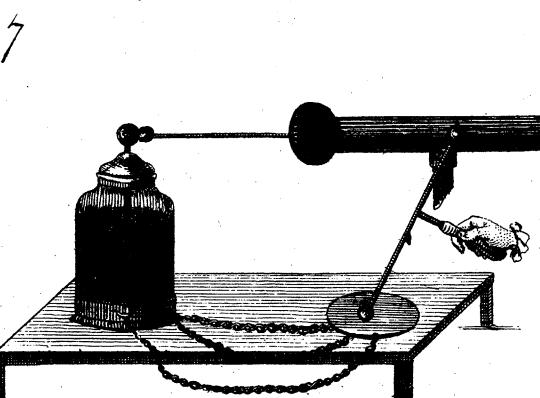
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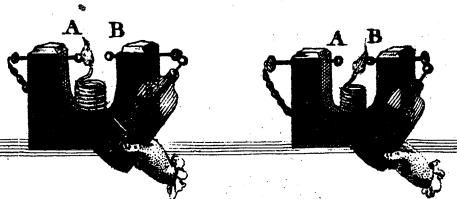
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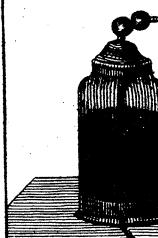
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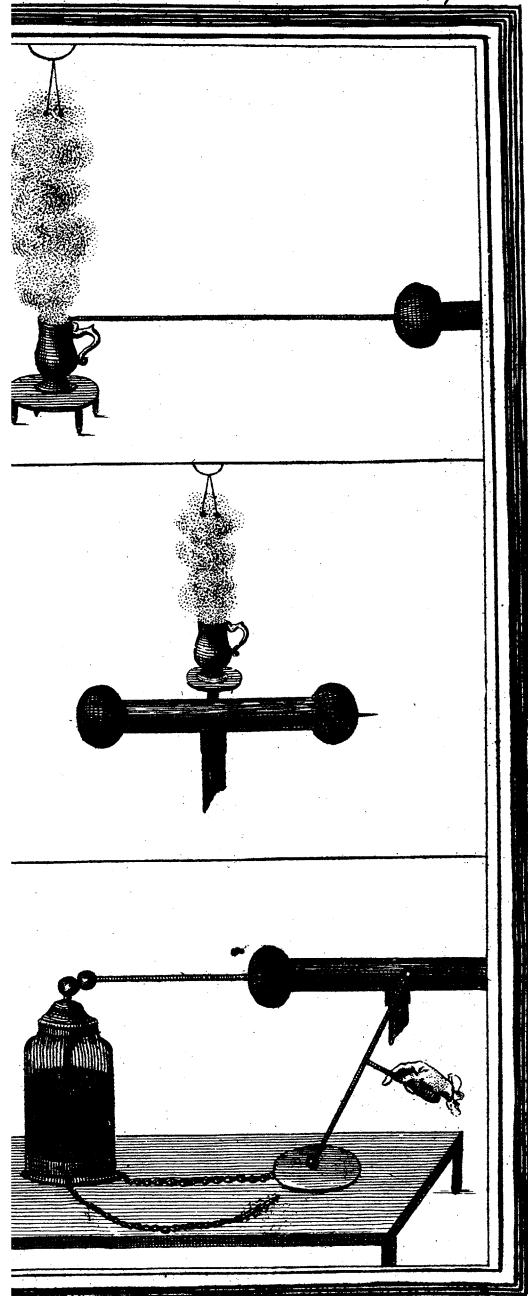
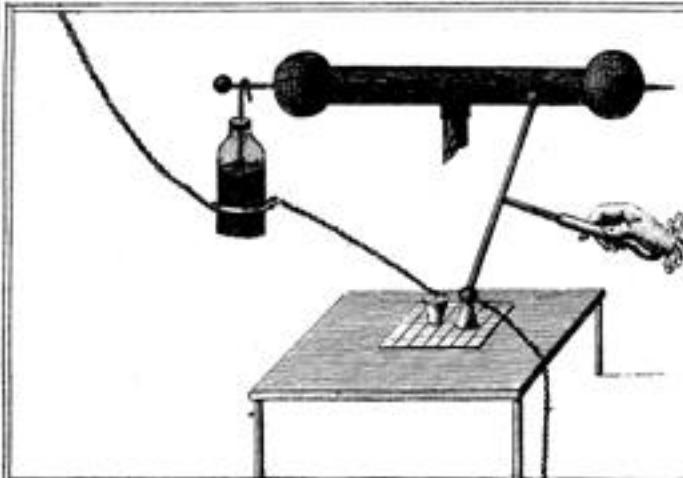


Fig. 1.



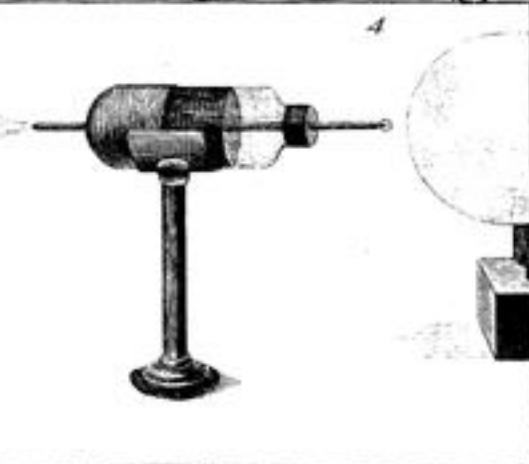
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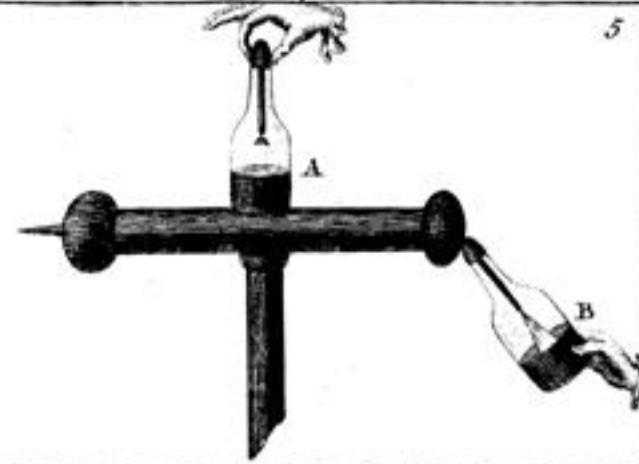
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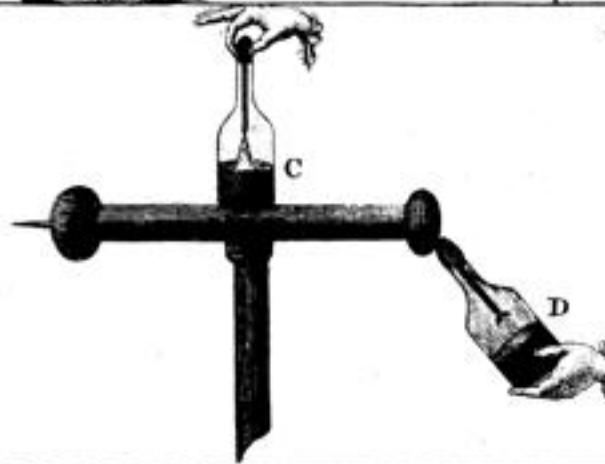
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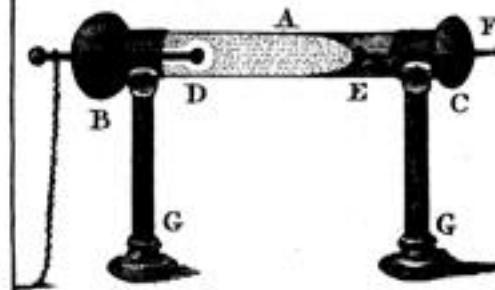
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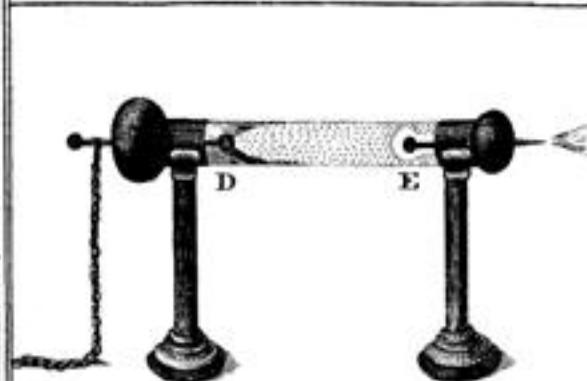
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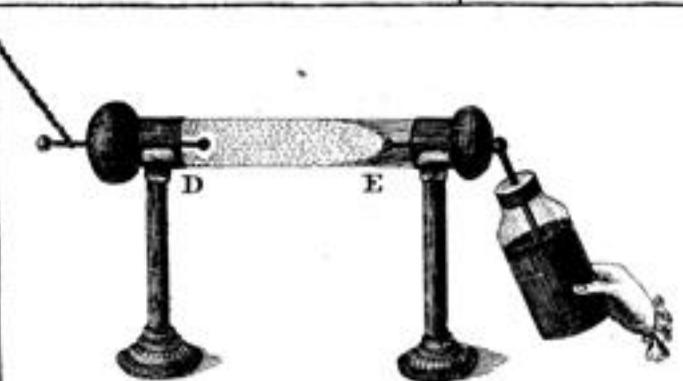
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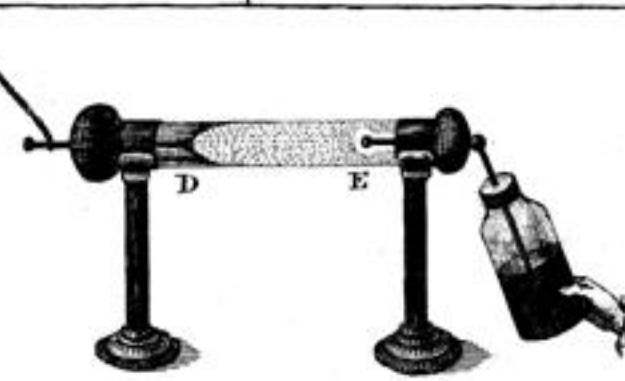


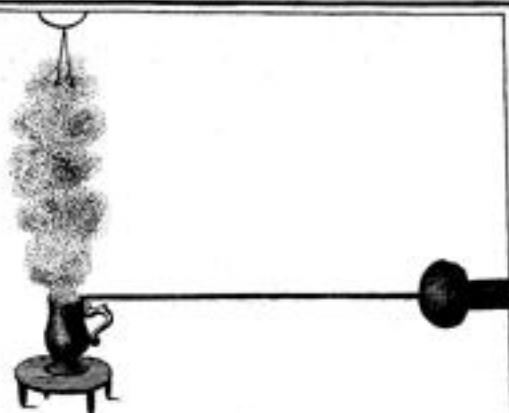
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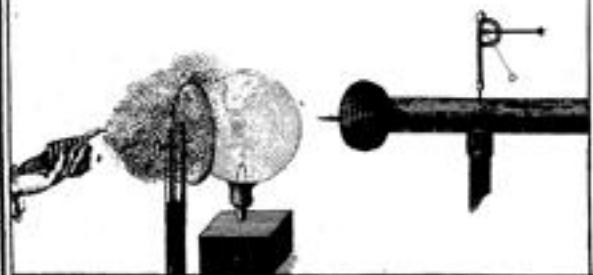
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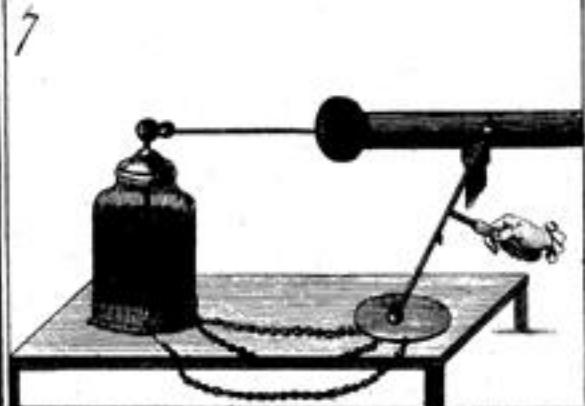
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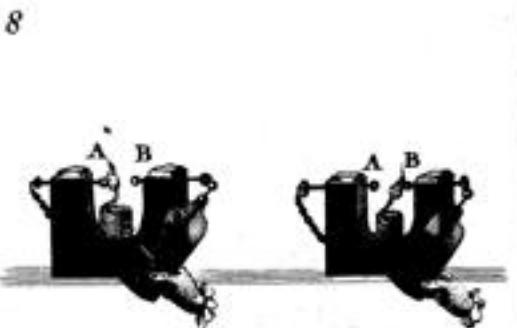
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